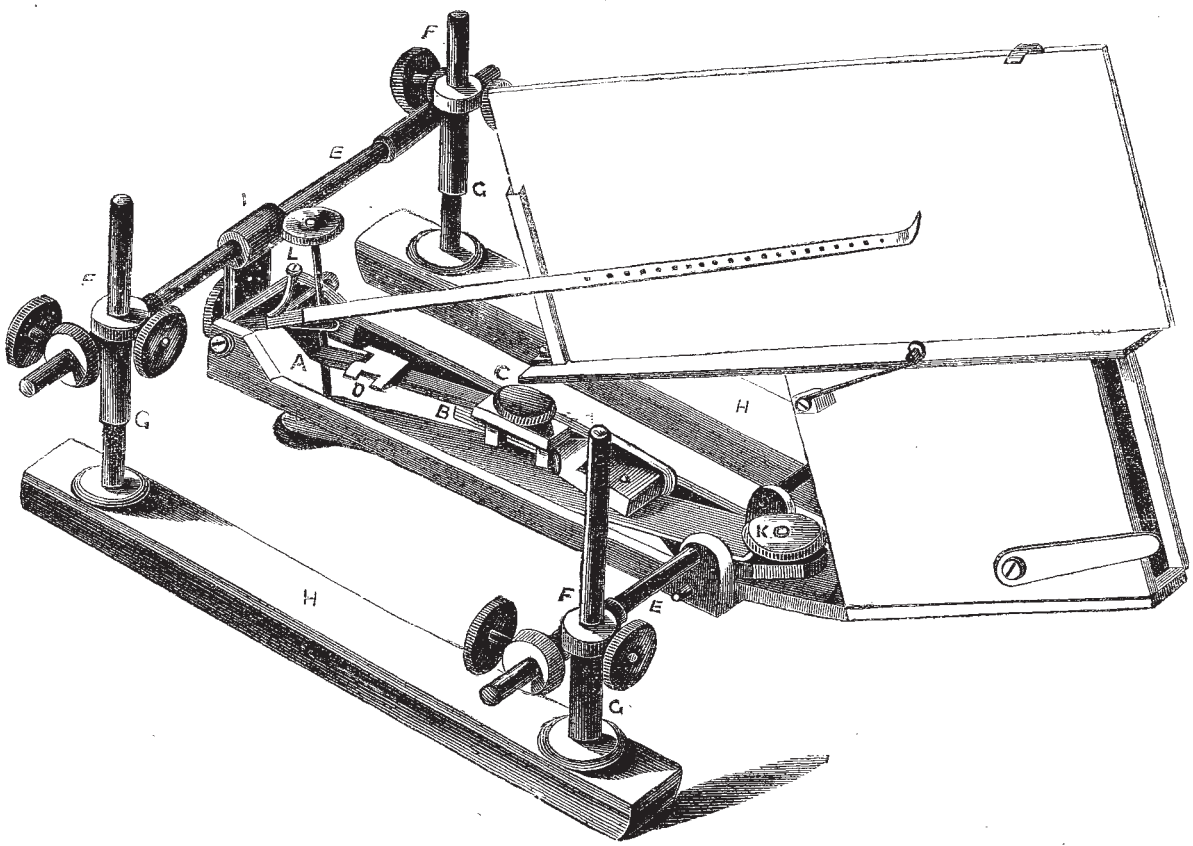
facturers are beginning to feel indifferent with regard to exhibitions, that the Vienna Exhibition in the past and the Philadelphia Exhibition in the present years, have been absorbing their energies, the Committee think that they have reason to be contented with the results obtained. This view is strengthened by comparing the above numbers with those of 127 exhibitors only who represented German science at Vienna. The Committee express themselves greatly obliged for the assistance given by the Lord President of the Council of Education, the Duke of Richmond and Gordon, the Vice-President, Viscount Sandon, the Director of the South Kensington Museum, Mr. Cunliffe Owen, and to the Government and Officers of the German Empire and of Prussia, notably to the Ministers of Education, Dr. Falk, of Commerce, Dr. Achenbach, of War, General von Kamecke, of Marine, to the head of the General Staff, Count Moltke, to the Postmaster-General, Dr. Stephan, and also to the Royal Library, to the Royal Academy of Science, and to the German Chemical Society. The Committee conclude by claiming the assistance of the German Empire for the production of a systematic and critical report on the scientific treasures of all nations that will be exhibited in London.

ON A MODIFIED CARDIOGRAPH

DR. A. L. GALABIN, whose investigations with the sphygmograph and cardiograph we have had the opportunity of noticing on former occasions (*vide* NATURE, vol. xii. p. 275), has introduced a modification of the cardiograph, a woodcut drawing of which, through the kindness of the Council of the Royal Medico-Chirurgical Society, we are able to reproduce from their "Transactions."

The cardiograph of Marey is too well known to require description; suffice it to say that it depends for its action on the transmission through air-filled tubes of movements from one stretched elastic membrane to another. In it, therefore, errors originating in the tubes are introduced; and these, from practical experience, are found to be considerable. More than one physiologist has obtained far more satisfactory "cardiograms" by applying the sphygmograph, which was originally constructed by its inventor—M. Marey—for the purpose of recording the movements of the pulse at the wrist, upon the chest-wall, in the intercostal spaces. This instrument, when thus



applied, reproduces in a most faithful manner the movements of the chest-walls as there produced by the subjacent heart in action; and in the healthy subject any accessory apparatus is rarely needed for the satisfactory production of the tracings.

In many pathological conditions, and in the healthy subject when the cardiac movements are more than ordinarily powerful, the movements of the heart are transmitted to the neighbouring ribs, on which the sphygmograph has to be supported, as well as to the more yielding intercostal tissues. Under these circumstances it is far better to employ, as supports for the instrument, more fixed points, which must, from the nature of the chest-wall, be at some distance from the centre of cardiac movement. Dr. Galabin's apparatus supplies us with the means needed. It is an expanded framework constructed in a manner which allows of its being firmly applied to a considerable expanse of the irregularly-shaped chest. From the drawing its principle can be best understood (See Figure).

In the middle of the figure the sphygmograph is seen. It differs from M. Marey's original in one or two minor details, which are decided improvements. The most important of these is that the brass bar A B, on which the

knife-edge by means of which the recording lever is set in motion is fixed, can be varied in length; and this makes it possible to vary the magnifying power of the lever, because the distance of the knife-edge from its axle can be changed. Such an addition has always been a desideratum, even in the wrist sphygmograph. The screw, C, clamps the two component parts in any desired position. A second reserve knife-edge, D, can also be turned up to replace the ordinary one, A, when the cardiac action is extraordinarily forcible. By the screw, K, the compress-spring is fixed. L is the secondary spring, which prevents the recording lever from quitting the knife-edge; it can be thrown out of gear when not required.

The supporting bars are seen at H H; they replacing the side-lappets of the original instrument. On them are fixed uprights, G G G, on which again are attached by screw-clamps two transverse bars for the suspension of the sphygmograph. That to which the clock-work end is joined can only be moved upon the uprights with which it is connected. The other has an additional sliding-piece, I, that allows of the screw-pad portion being independently raised or lowered in a hinged manner.

That this suspending stage will prove of great service in the study of the heart's action there can be no doubt. The presence of the large number of movable centres must, however, render its adjustment somewhat difficult. It will be seen in the figure that the recording plate above the watch-work is of considerable depth. We have found, practically, that it is *never* advantageous to allow the oscillations of the lever to reach nearly so great an amplitude as this will permit; and it is known by all that it is very important that the average level of the lever's tracing should never be far above that line which is perpendicular to the tangent of the circle formed by the lever in its movements, at the point where the two cut one another.

Whilst on the subject of Dr. Galabin's cardiograph and sphygmograph work, we may incidentally draw attention to a point in a paper by him in the January number of the *Journal of Anatomy and Physiology*. Dr. Galabin there comments on Mr. Garrod's law respecting the length of the cardiac systole as it appears in the arterial system—that it is constant for any given pulse-rate, and varies as the cube root of the rate. He remarks, "I have found the length of the systolic portion of the pulse-curve to deviate somewhat considerably from that deduced from the equation. . . . It appears to be approximately true in normal pulses." Would it not have been better if Dr. Galabin had given a larger number of examples—he having confined himself to two, of which one is pathological? In the paper in which the law was announced, the agreement of the measurements with the requirements was very close, and others have been published since, even more satisfactory; it has also been indicated by its author that a pathological condition, like anæmia (the instance taken), is just such an one as that in which a deviation might be expected. Mr. Edgar Thurston, of King's College, has recently read a paper before the Medical Society of that School, which is quite in confirmation of the law as originally stated, from a considerable number of observations on *healthy* subjects.

PHYSICAL SCIENCE IN SCHOOLS

DR. WATTS quite puzzles me. I can see no contradiction between the passages from my essay of 1867 and my letter of 1876, which he silently places in juxtaposition. What I said in 1867 was (p. 261) that "science should be introduced into a school, beginning at the top and going downwards gradually to a point which will be indicated by experience." What I say in 1876 is that experience shows, as far as I can judge, that it is not generally wise to go down very far; that one soon comes to a point at which the loss in teaching science counterbalances the gain. I am quite as sure as ever I was of the value of science in schools, in its right place.

I think that those who advocate the teaching of science to young boys scarcely realise the difficulty of establishing their ground. Some, like Prof. Roscoe (p. 387), admit, when pressed, that it is a question which experience alone can decide, and that they have not had that experience. Liebig, to whom Mr. Gerstl refers (p. 431), was speaking of a different class of schools, in which boys must pick up some useful scientific facts early or not at all. Prof. Henslow's experience is of the same kind. Other philosophers, charmed with the bright intelligence of children when talked to by a Faraday or a Frankland, straightway pronounce an opinion on the relative value of science and classics and mathematics in the early part of a liberal education,—on somewhat insufficient grounds.

The question that this discussion began with was the merits or demerits of the Certificate Examination, in so far as it affected science in schools. That seems to be settled. We have drifted now into a different and most useful discussion on the results of experience in the early teaching of science. The question is this. Given that boys are going to remain under a system of liberal education till eighteen or nineteen, at what stages is it shown by experience that it is wise to introduce the different sciences? It is a question of the *comparative* value of different studies at different ages, not only of what may best be learnt, but of what may least injuriously remain unlearnt, at different ages; and those teachers speak with real weight who can institute such a comparison; men who have watched the processes by which young boys learn different subjects. A man who teaches science only cannot institute such a comparison. He can only say, "I *do* teach young boys something of chemistry and botany, and they *do* gain something." One who teaches mathematics also is so far better off that he can say, "Young boys are more (or less) attentive, active-minded, diligent, when they are doing arithmetic, than when they are at a lesson on physical geography; and they are more (or less) incapable in later years of recovering from the ill effects of neglected arithmetic than of neglected physical geography." One who teaches classics also (as I do for more hours a week than I teach physical science) has wider grounds still for forming a comparison.

Nothing that I see young boys do is as efficient as Latin in completely occupying their minds with perpetually recurring problems which tax attention, memory, judgment, taste. It is quite interesting enough not to be too tiresome. The problems are easy and varied, and the solutions certain and satisfactory. The same sort of young boys who will work hard and cheerfully over a bit of Cæsar or a Latin exercise seem to be a good deal bored by a lesson on physical geography, think botany rather nonsense, and submit silently to the hopeless unintelligibility of "matter and motion." The very same boys will as a rule enjoy an arithmetic lesson and work happily at their practical geometry, or, when well handled, their Euclid. Hence, if I wanted to train up a boy for a scientific career, I would not begin very early with science, but wait till he was thirteen or fourteen.

I admit that the experience of some others is against me.

Mr. Tuckwell (p. 412) speaks warmly, and pronounces my opinion to mean nothing more than that I myself have failed to teach science to young boys. This is a mistake. It means that I have seen the work of others, here and elsewhere. It means not an absolute failure, but a comparative failure, as explained above. It means a summary of the opinions of a considerable number of other men. Mr. Wyles (p. 455) is against me, although he has "never been satisfied with his science teaching." Mr. West (vol. xiii. p. 48) is against me, and his opinion